MR. CRUDGINGTON, of the Baptist Congo Expedition, has just returned to England for a short time, and reports that by now Messrs. Comber and Bentley will have formed a first station at Isangila, Mr. Stanley's second post, and will at once push on to Mbu, some sixty or seventy miles further along the north bank of the Congo, where the the second station will be erected. It is for this navigable portion of the river that a steel boat is required, so as to avoid the Basundi. In his late journey up the Congo Mr. Crudgington found these people warlike and trouble-some, as Mr. Stanley had done, and they were a source of per-petual anxiety to him. The practicability of utilising this part of the river is shown by the fact that Mr. Crudgington and his party went over it in heavy, clumsy, native canoes; but from Mbu the expedition will have to go to Stanley Pool by land, as the rapids and falls render the river quite unnavigable. The plans of the expedition are now on such an extended scale that six additional missionaries will be required—two for San Salva-dor, one for the depot at Mussuca on the Lower Congo, one each for Isangila and Mbu, and four for Stanley Pool, so that occasional journeys may be made higher up the Congo. The steel boat required by the expedition has been presented by an anonymous donor at a cost of about 400*l.* It has been built in London from drawings furnished by Mr. H. M. Stanley.

THE *Times* last week published an exceedingly interesting letter on trade and exploration on the Congo from a private cor-respondent at the mouth of the Ogowé. Speaking of M. Savorgnan de Brazza, the writer says that he has done much to open up the country between the Ogowé and the Congo, that he purchased a large tract of country near the sources of the former river at a very cheap rate, erected a station, and left a white man in charge. He is said to have purchased villages as they stood, freed a great many slaves, and engaged them at monthly wages to cultivate the plantations and keep the ground in order. He seems to have been regarded as the apostle of freedom in the country; troops of slaves came flocking to him to be freed, and his visit is regarded as having struck a blow at slavery in West Africa. The writer gives a very different picture of the state of affairs on the Belgian road along the north bank of the Congo. It may be interesting to mention, the observations respecting the light in which M. de Brazza is viewed by the natives are fully confirmed by a letter from a Roman Catholic missionary who accompanied him up the Ogowé last December.

WHIRLED ANEMOMETERS

IN the course of the year 1872 Mr. R. H. Scott, F.R.S., suggested to the Meteorological Committee the desirability of carrying out a series of experiments on anemometers of dif-ferent patterns. This suggestion was approved by the Com-mittee, and in the course of the same year a grant was obtained by Mr. Scott from the Government grant administered by the Royal Society for the purpose of defraying the expenses of the investigation. The experiments were not however carried out by Mr. Scott himself, but were intrusted to Mr. Samuel Jeffery, then Superintendent of the Kew Observatory, and Mr. G. M. Whipple, then First Assistant, the present Superintendent.

The results have never hitherto been published, and I was not aware of their nature till on making a suggestion that an anemo-meter of the Kew Standard pattern should be whirled in the open air, with a view of trying that mode of determining its proper factor, Mr. Scott informed me of what had already been done, and wrote to Mr. Whipple, requesting him to place in my hands the results of the most complete of the experiments, namely, those carried on at the Crystal Palace, which I accord-ingly obtained from him. The progress of the inquiry may be gathered from the following extract from Mr. Scott's report in returning the unexpended balance of the grant:—

" The comparisons of the instruments tested were first insti-tuted in the garden of the Kew Observatory. This locality was found to afford an insufficient exposure.

" A piece of ground was then rented and inclosed within the Old Deer Park. The experiments here showed that there was a considerable difference in the indications of anemometers of different sizes, but it was not possible to obtain a sufficient range of velocities to furnish a satisfactory comparison of the instru-ments. Experiments were finally made with a rotating appara-tus, a steam merry-go-round, at the Crystal Palace, which led to

some results similar to those obtained by exposure in the Deer Park.

"The subject has however been taken up so much more thoroughly by Doctors Dohrandt and Thiesen (*vide* "Repertorium für Meteorologie," vols. iv. and v.), and by Dr. Robinson in Dublin, that it seems unlikely that the balance would ever be expended by me. I therefore return it with many thanks to the Government Grant Committee.

"The results obtained by me were hardly of sufficient value to be communicated to the Society."

On examining the records it seemed to me that they were well deserving of publication, more especially as no other experiments of the same kind have, so far as I know, been executed on an anemometer of the Kew standard pattern. In 1860 Mr. Glaisher made experiments with an anemometer whirled round in the open air at the end of a long horizontal pole,[2] but the anemo-meter was of the pattern employed at the Royal Observatory, with hemispheres of 3·75 inches diameter and arms of 6·725 inches, measured from the axis to the centre of a cup, and so was considerably smaller than the Kew pattern. The experi-ments of Dr. Dohrandt and Dr. Robinson were made in a building, which has the advantage of sheltering the anemometer from wind, which is always more or less fitful, but the disadvantage of creating an eddying vorticose movement in the whole mass of air operated on; whereas in the ordinary employment of the anemometer the eddies it forms are carried away by the wind, and the same is the case to a very great extent when an anemo-meter is whirled in the open air in a gentle breeze. Thus, though Dr. Robinson employed among others an anemometer of the Kew pattern, his experiments and those of Mr. Jeffery are not duplicates of each other, even independently of the fact that the axis of the anemometer was vertical in Mr. Jeffery's and hori-zontal in Dr. Robinson's experiments; so that the greater com-pleteness of the latter does not cause them to supersede the former.

In Mr. Jeffery's experiments the anemometers operated on were mounted a little beyond and above the outer edge of one of the steam merry-go-rounds in the grounds of the Crystal Palace, so as to be as far as practicable out of the way of any vortex which it might create. The distance of the axis of the anemometer from the axis of the "merry" being known, and the number of revolutions (*n*) of the latter during an experiment counted, the total space traversed by the anemometer was known. The number (N) of *apparent* revolutions of the anemometer, that is, the number of revolutions *relatively to the merry*, was recorded on a dial attached to the anemometer, which was read at the beginning and end of each experiment. As the machine would only go round one way the cups had to be taken off and replaced in a reverse position, in order to reverse the direction of revolu-tion of the anemometer. The *true* number of revolutions of the anemometer was, of course, N + *n*, or N − *n*, according as the rotations of the anemometer and the machine were in the same or opposite directions.

The horizontal motion of the air over the whirling machine during any experiment was determined from observations of a dial anemometer with 3-inch cups on 8-inch arms, which was fixed on a wooden stand in the same horizontal plane as that in which the cups of the experimental instrument revolved, at a distance estimated at about 30 feet from the outside of the whirling frame. The motion of the centres of the cups was deduced from the readings of the dial of the fixed anemometer at the beginning and end of each experiment, the motion of the air being assumed as usual to be three times that of the cups.

The experiments were naturally made on fairly calm days, still the effect of the wind, though small, is not insensible. In default of further information, we must take its velocity as equal to the mean velocity during the experiment.

Let V be the velocity of the anemometer, W that of the wind, *θ* the angle between the direction of motion of the anemometer and that of the wind. Then the velocity of the anemometer relatively to the wind will be—

$$\sqrt{V^2 - 2VW \cos \theta + W^2} \quad \ldots \quad \ldots \quad (a)$$

The mean effect of the wind in a revolution of the merry will be different according as we suppose the moment of inertia of the anemometer very small or very great.

If, as is practically the case, W be small as compared with V, the correction to be added to V on account of the wind may be

[1] " Discussion of the Results of some Experiments with Whirled Anemo-meters." Paper read at the Royal Society, May 12, by Prof. G. G. Stokes, Sec. R.S.

[2] "Greenwich Magnetical and Meteorological Observations," 1862, Intro-duction, p. li.

shown to be $W^2/4V$ on the first supposition, and $3W^2/4V$ on the second.

Three anemometers were tried, namely, one of the old Kew standard pattern, one by Adie, and Kraft's portable anemometer. Their dimensions, &c., were as follows :—

(α) *The Old Kew Standard.*—Diameter of arms between centres of cups 48 inches ; diameter of cups 9 inches. Fixed to machine at 22·3 feet from the axis of revolution.

(β) *Adie's Anemometer.*—Diameter of arms between centres of cups 13·4 inches ; diameter of cups 2·5 inches. Fixed to machine at 20·7 feet from the axis of revolution.

(γ) *Kraft's Portable Anemometer.*—Diameter of arms between centres of cups 8·3 inches ; diameter of cups 3·3 inches. Fixed to machine at 19·10 feet from the axis of revolution.

With each anemometer the experiments were made in three groups, with high, moderate, and low velocities respectively, averaging about 28 miles an hour for the high, 14 for the moderate, and 7 for the low. Each group again was divided into two subordinate groups, according as the cups were direct, in which case the directions of rotation of the merry and of the anemometer were opposite, or reversed, in which case the directions of the two rotations were the same.

The data furnished by each experiment were : the time occupied by the experiment, the number of revolutions of the merry, the number of *apparent* revolutions of the anemometer, given by the difference of readings of the dial at the beginning and end of the experiment, and the space S passed over by the wind, deduced from the difference of readings of the fixed anemometer at the beginning and end of the experiment.

The object of the experiment was of course to compare the mean velocity of the centres of the cups with the mean velocity of the air relatively to the anemometer. It would have saved some numerical calculation to have compared merely the spaces passed through during the experiment ; but it seemed better to exhibit the velocities in miles per hour, so as to make the experiments more readily comparable with one another, and with those of other experimentalists. In the reductions I employed 4-figure logarithms, so that the last decimal in V in the tables cannot quite be trusted, but it is retained to match the correction for W., which it seemed desirable to exhibit to 0·01 mile.

On reducing the experiments with the low velocities I found the results extremely irregular. I was subsequently informed by Mr. Whipple that the machine could not be regulated at these low velocities, for which it was never intended, and that it sometimes went round fast, sometimes very slowly. He considered that the experiments in this group were of little, if any, value, and that they ought to be rejected. They were besides barely half as numerous as those of the moderate group. I have accordingly thought it best to omit them altogether.

In the complete paper tables are then given containing the reduced results of the individual experiments, and from them the mean results for the high and moderate velocities are collected in the following table, in which are also inserted the mean errors :—

Anemometer.	Directions of rotation	High velocities.				Moderate velocities.			
		Mom. inert. small.		Mom. inert. large.		Mom. inert. small.		Mom. inert. large.	
		p. c.	m. e.	p. c.	m. e.	p. c.	m. e.	p. c.	m. e.
Kew.	Opposite ...	122·6	2·4	121·9	2·3	115·1	4·9	113·2	5·2
	Alike	118·4	2·9	117·5	2·8	109·7	4·5	108·5	5·1
	Mean ...	120·5	...	119·7	...	112·4	...	110·8	
Adie.	Opposite ...	95·1	2·3	94·2	2·3	88·5	4·5	86·8	5·0
	Alike	98·0	6·5	97·3	6·5	82·6	7·3	81·0	7·3
	Mean ...	96·5	...	95·7	...	85·5	...	83·9	
Kraft.	Opposite ...	101·5	2·6	100·8	2·5	89·1	4·8	86·9	5·1
	Alike	100·8	1·2	99·4	1·3	87·8	5·0	86·0	6·0
	Mean ...	101·1	...	100·1	...	88·4	...	86·4	

The mean errors exhibited in the above table show no great difference according as we suppose the moment of inertia of the anemometer small or large in correcting for the wind. From the mean errors we may calculate nearly enough, by the usual formulæ, the probable errors of the various mean percentages for rotations opposite and alike. The probable errors of these mean percentages come out as follows :—

Kew, 1·0 for high velocities ; 2·7 for moderate velocities.
Adie, 1·5 ,,   ,,   2·0   ,,   ,,
Kraft, 0·9 ,,   ,,   1·8   ,,   ,,

These probable errors are so small that it appears that for the high and even for the moderate velocities the experiments are extremely trustworthy, except in so far as they may be affected by *systematic* sources of error.

It may be noticed that the difference of the percentages according as the directions of rotation of the anemometer and of the merry are opposite or alike is greatest for the Kew, in which the ratio of r to R is greatest, r denoting the radius of the arm of the anemometer, and R the distance of its axis from the axis of revolution of the machine, and appears to be least (when allowance is made for the two anomalous experiments in the group " Adie H + ") for the Kraft, for which r/R is least. In the Kraft indeed the differences are roughly equal to the probable errors of the means. In these whirling experiments r/R is always taken small, and we might expect the correction to be made on account of the finiteness of R to be expressible in a rapidly converging series according to powers of r/R, say—

$$A' \frac{r}{R} + B'\left(\frac{r}{R}\right)^2 + C'\left(\frac{r}{R}\right)^3 + \cdots$$

We may in imagination pass from the case of rotations opposite to that of rotations alike, by supposing R taken larger and larger in successive experiments, altering the angular velocity of revolution so as to preserve the same linear velocity for the anemometer, and supposing the increase continued until R changes sign in passing through infinity, and is ultimately reduced in magnitude to what it was at first. The ideal case of R = ∞ is what we aim at, in order to represent the motion of a fixed anemometer acted on by perfectly uniform wind by that of an anemometer uniformly impelled in a rectilinear direction in perfectly still air. We may judge of the magnitude of the leading term in the above correction, provided it be of an odd order, by that of the difference of the results for the two directions of rotation. Unless therefore we had reason to believe that A' were 0, or at least very small compared with B', we should infer that the whole correction for the finiteness of R is very small, and that it is practically eliminated by taking the mean of the results for rotations opposite and rotations alike.

We may accept, therefore, the mean results as not only pretty well freed from casual irregularities which would disappear in the mean of an infinite number of experiments, but also, most probably, from the imperfection of the representation of a rectilinear motion of the anemometer·by motion in a circle of the magnitude actually employed in the experiments.

Before discussing further the conclusions to be drawn from the results obtained, it will be well to consider the possible influence of systematic sources of error.

1. *Friction.*—No measure was taken of the amount of friction, nor were any special appliances used to reduce it ; the anemometers were mounted in the merry just as they are used in actual registration. Friction arising from the weight is guarded against as far as may be in the ordinary mounting, and what remains of it would act alike in the ordinary use of the instrument and in the experiments, and as far as this goes, therefore, the experiments would faithfully represent the instrument as it is in actual use. But the bearings of an anemometer have also to sustain the lateral pressure of the wind, which in a high wind is very considerable ; and the construction of the bearing has to be attended to in order that this may not produce too much friction. So far the whirled instrument is in the same condition as the fixed. But besides the friction arising from the pressure of the artificial wind, a pressure which acts in a direction tangential to the circular path of the whirled anemometer, there is the pressure arising from the centrifugal force. The highest velocity in the experiments was about thirty miles an hour, and at this rate the centrifugal force would be about three times the weight of the anemometer. This pressure would considerably exceed the former, at right angles to which it acts, and the two would compound into one equal to the square root of the sum of their squares. The resulting friction would exceed a good deal that arising from the pressure of the wind in a fixed anemometer with the same velocity of wind, natural or artificial, and would

sensibly reduce the velocity registered, and accordingly raise the coefficient which Dr. Robinson denotes by *m*, the ratio, namely, of the velocity of the wind to the velocity of the centres of the cups. It may be noticed that the percentages collected in the above table are very distinctly lower for the moderate velocities than for the high velocities. Such an effect would be produced by friction; but how far the result would be modified if the extra friction due to the centrifugal force were got rid of, and the whirled anemometer thus assimilated to a fixed anemometer, I have not the means of judging, nor again how far the percentages would be still further raised if friction were got rid of altogether.

Perhaps the best way of diminishing friction in the suppport of an anemometer is that devised and employed by Dr. Robinson, in which the anemomeser is supported near the top on a set of spheres of gun-metal contained in a box with a horizontal bottom and vertical side which supports and confines them. For vertical support this seems to leave nothing to be desired, but when a strong lateral pressure has to be supported as well as the weight of the instrument, it seems to me that a slight modification of the mode of support of the balls might be adopted with advantage. When a ball presses on the bottom and vertical side of its box, and is at the same time pressed down by the horizontal disk attached to the shaft of the anemometer which rests on the balls, it revolves so that the instantaneous axis is the line joining the points of contact with the fixed box. But if the lateral force of the wind presses the shaft against the ball the ball cannot simply roll as the anemometer turns round, but there is a slight amount of rubbing.

This however may be obviated by giving the surfaces where the ball is in contact other than vertical or horizontal direction.

Let A B be a portion of the cylindrical shaft of an anemometer; C D the axis of the shaft; E F G H I a section of the fixed box or cup containing the balls; L M N a section of a conical surface fixed to the shaft by which the anemometer rests on its balls; F I K M a section of one of the balls; F, I, the points of contact
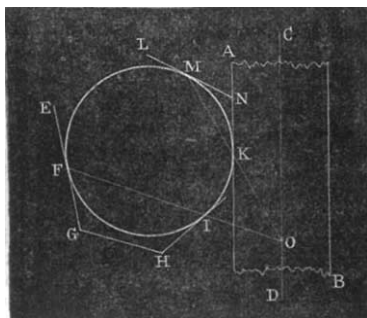
of the ball with the box; M the point of contact with the supporting cone; K the point of contact, or all but contact, of the ball with the shaft. The ball is supposed to be of such size that when the anemometer simply rests on the balls by its own weight, being turned perhaps by a gentle wind, there are contacts at the points M, F, I, while at K the ball and shaft are separated by a space which may be deemed infinitesimal. Lateral pressure from a stronger wind will now bring the shaft into contact with the ball at the point K also, so that the box on the one hand and the shaft with its appendage on the other will bear on the ball at four points. The surface of the box, as well as that on the cone L N, being supposed to be one of revolution round C D, those four points will be situated in a plane through C D, which will pass of course through the centre of the ball.

If the ball rolls without rubbing at any one of the four points F, I, K, M as the anemometer turns round, its instantaneous axis must be the line joining the points of contact F, I, with the fixed box. But as at M and K there is nothing but rolling, the instantaneous motion of the ball may be thought of as one in which it moves as if it were rigidly connected with the shaft and its appendage, combined with a rotation over L N A B supposed fixed. For the two latter motions the instantaneous axes are C D, M K respectively. Let M K produced cut C D in O. Then since the instantaneous motion is compounded of rotations round two axes passing through O, the instantaneous axis must pass through O. But this axis is F I. Therefore F I must pass through O. Hence the two lines F I, M K must intersect the axis of the

shaft in the same point, which is the condition to be satisfied in order that the ball may roll without rubbing, even though impelled laterally by a force sufficient to cause the side of the shaft to bear on it. The size of the balls and the inclinations of the surfaces admit of considerable latitude subject to the above condition. The arrangement might suitably be chosen something like that in the figure. It seems to me that a ring of balls constructed on the above principle would form a very effective upper support for an anemometer whirled with its axis vertical. Possibly the balls might get crowded together on the outer side by the effect of centrifugal force. This objection, should it be practically found to be an objection, would not of course apply to the proposed system of mounting in the case of a fixed anemometer. Below, the shaft would only require to be protected from lateral motion, which could be done either by friction wheels or by a ring of balls constructed in the usual manner, as there would be only three points of contact.

2. *Influence on the Anemometer of its own Wake.*—By this I do not mean the influence which one cup experiences from the wake of its predecessor, for this occurs in the whirling in almost exactly the same way as in the normal use of the instrument, but the motion of the air which remains at any point of the course of the anemometer in consequence of the disturbance of the air by the anemometer when it was in that neighbourhood in the next preceding and the still earlier revolutions of the whirling instrument.

It seems to me that in the open air, where the air impelled by the cups is free to move into the expanse of the atmosphere, instead of being confined by the walls of a building, this must be but small, more especially as the wake would tend to be carried away by what little wind there might be at the time. On making some inquiries from Mr. Whipple as to a possible vorticose movement created in the air through which the anemometer passed, he wrote as follows:—" I feel confident that under the circumstances the tangential motion of the air at the level of the cups was so small as not to need consideration in the discussion of the results. As in one or two points of its revolution the anemometer passed close by some small trees in full leaf, we should have observed any eddies or artificial wind had it existed, but I am sure we did not."

3. *Influence of the Variation of the Wind; first, as regards Variations which are not Rapid.*—During the twenty or thirty minutes that an experiment lasted there would of course be numerous fluctuations in the velocity of the wind, the mean result of which is alone recorded. The period of the changes (by which expression it is not intended to assert that they were in any sense regularly periodic), might be a good deal greater than that of the merry, or might be comparatively short. In the high velocities, at any rate, in which one revolution took only three or four seconds, the supposition that the period of the changes was large compared with one revolution is probably a good deal nearer the truth than the supposition that it is small.

On the former supposition the correction for the wind during two or three revolutions of the merry would be given by the formulæ already employed, taking for W its value at the time. Consequently the total correction will be given by the formulæ already used if we substitute the mean of W^2 for the square of mean W. The former is necessarily greater than the latter, but how much we cannot tell without knowing the actual variations. We should probably make an outside estimate of the effect of the variations if we supposed the velocity of the wind twice the mean velocity during half the duration of the experiment, and nothing at all during the remainder. On this supposition the mean of W^2 would be twice the square of mean W, and the correction for the wind would be doubled. At the high velocities of revolution, the whole correction for the wind is so very small that the uncertainty arising from variation as above explained is of little importance, and even for the moderate velocities it is not serious.

4. *Influence of Rapid Variations of the Wind.*—Variations of which the period is a good deal less than that of the revolutions of the whirling instrument act in a very different manner. The smallness of the corrections for the wind hitherto employed depends on the circumstance that with uniform wind, or even with variable wind, when the period of variation is a good deal greater than that of revolution of the merry, the terms depending on the first power of W, which letter is here used to denote the momentary velocity of the wind, disappear in the mean of a revolution. This is not the case when a particular velocity of wind belongs only to a particular part of the circle described by

the anemometer in one revolution. In this case there will in general be an outstanding effect depending on the first power of W, which will be considerably larger than that depending on W². Thus suppose the velocity of whirling to be thirty miles an hour, and the average velocity of the wind three miles an hour, the correction for the wind supposed uniform, or if variable, then with not very rapid variations, will be comparable with 1 per cent. of the whole; whereas, with rapid variations, the effect in any one revolution may be comparable with 10 per cent. There is, however, this important difference between the two: that whereas the correction depending on the square leaves a positive residue, however many experiments be made, the correction depending on the first power tends ultimately to disappear, unless there be some cause tending to make the average velocity of the wind different for one azimuth of the whirling instrument from what it is for another. This leads to the consideration of the following conceivable source of error.

5. *Influence of Partial Shelter of the Whirling Instrument.*— On visiting the merry-go-round at the Crystal Palace, I found it mostly surrounded by trees coming pretty near it, but in one direction it was approached by a broad open walk. The consequence is that the anemometer may have been unequally sheltered in different parts of its circular course, and the circumstances of partial shelter may have varied according to the direction of the wind. This would be liable to leave an uncompensated effect depending on the first power of W. I do not think it probable that any large error was thus introduced, but it seemed necessary to point out that an error of the kind may have existed.

The effect in question would be eliminated in the long run if the whirling instrument were capable of reversion, and the experiments were made alternately with the revolution in one direction, and the reverse. For then, at any particular point of the course at which the anemometer was more exposed to wind than on the average, the wind would tend to increase the velocity of rotation of the anemometer for one direction of revolution of the whirling instrument just as much, ultimately, as to diminish it for the other. Mere reversion of the cups has no tendency to eliminate the error arising from unequal exposure in different parts of the course. And even when the whirling instrument is capable of reversion. it is only very slowly that the error arising from partial shelter is eliminated compared with that of irregularities in the wind; of those irregularities, that is to say, which depend on the first power of W. For these irregularities go through their changes a very great number of times in the course of an experiment lasting perhaps half an hour, whereas the effect of partial shelter acts the same way all through one experiment. It is very desirable therefore that in any whirling experiments carried on in the open air, the condition of the whirling instrument as to exposure or shelter should be the same all round.

The trees, though taller than the merry when I visited the place last year, were but young, and must have been a good deal lower at the time that the experiments were made. Mr. Whipple does not think that any serious error is to be apprehended from exposure of the anemometer during one part of its course and shelter during another.

From a discussion of the foregoing experiments it seems to me that the following conclusions may be drawn :—

1. That, at least for high winds, the method of obtaining the factor for an anenometer, which consists in whirling the instrument in the open air, is capable, with proper precautions, of yielding very good results.

2. That the factor varies materially with the pattern of the anemometer. Among those tried, the anemometers with the larger cups registered the most wind, or in other words required the lowest factors to give a correct result.

3. That with the large Kew pattern, which is the one adopted by the Meteorological Office, the register gives about 120 per cent. of the truth, requiring a factor of about 2·5, instead of 3. Even 2·5 is probably a little too high, as friction would be introduced by the centrifugal force, beyond what occurs in the normal use of the instrument.

4. That the factor is probably higher for moderate than for high velocities; but whether this is solely due to friction the experiments do not allow us to decide.

Qualitatively considered, these results agree well with those of other experimentalists. As the factor depends so much on the pattern of the anemometer it is not easy to find other results with which to compare the actual numbers obtained, except in the case the Kew standard. The results obtained by Dr. Robinson, by

rotating an anemometer of this pattern without friction purposely applied are given at pp. 797 and 799 of the *Phil. Trans.* for 1878. The mean of a few taken with velocities of about 27 miles an hour in still air gave a factor 2·36, instead of 2·50, as got from Mr. Jeffery's experiments. As special anti-friction appliances were used by Dr. Robinson, the friction in Mr. Jeffery's experiments was probably a little higher. If such were the case the factor ought to come out a little higher than in Dr. Robinson's experiments, which is just what it does. As the circumstances of the experiments were widely different with respect to the vorticose motion of the air produced by the action of the anemometer in it, we may, I think, conclude that no very serious error is to be apprehended on this account.

In a later paper (*Phil. Trans.* for 1880, p. 1055), Dr. Robinson has determined the factor for an anemometer (among others) of the Kew pattern by a totally different method, and has obtained values considerably larger than those given by the former method. Thus the limiting value of the factor *m* corresponding to very high velocities, is given at p. 1063 as 2·826, whereas the limiting value obtained by the former method was only 2·286. Dr. Robinson has expressed a preference for the later results. I confess I have always been disposed to place greater reliance on the results of the Dublin experiments, which were carried out by a far more direct method, in which I cannot see any flaw likely to account for so great a difference. It would be interesting to try the second method in a more favourable locality.

I take this opportunity of putting out some considerations respecting the general formula of the anemometer, which may perhaps not be devoid of interest.

The problem of the anemometer may be stated to be as follows :—Let a uniform wind with velocity V act on a cup anemometer of given pattern, causing the cups to revolve with a velocity *v*, referred to the centre of the cups, the motion of the cups being retarded by a force of friction F ; it is required to determine *v* as a function of V and F, F having any value from 0, corresponding to the ideal case of a frictionless anemometer, to some limit F_1, which is just sufficient to keep the cups from turning. I will refer to my appendix to the former of Dr. Robinson's papers (*Phil. Trans.* for 1878, p. 818), for the reasons for concluding that F is equal to V², multiplied by a function of V/*v*. Let

$$V/v = \xi, \qquad F/V^2 = \eta,$$

then if we regard ξ and η as rectangular co-ordinates we have to determine the form of the curve, lying within the positive quadrant $\xi O \eta$, which is defined by those co-ordinates.
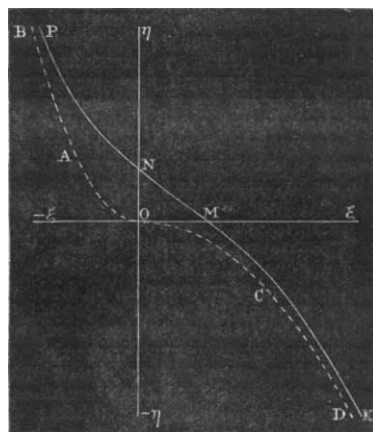
We may regard the problem as included in the more general problem of determining *v* as a function of V and F, where V is positive, but F may be of any magnitude and sign, and therefore *v* also.[1] Negative values of F mean, of course, that the cups, instead of being retarded by friction, are acted on by an impelling force making them go faster than in a frictionless anemometer, and values greater than F_1 imply a force sufficient to send them round with the concave sides foremost.

[1] Of course *v* must be supposed not to be so large as to be comparable with the velocity of sound, since then the resistance to a body impelled through air, or having air impinging on it, no longer varies as the square of the velocity.

Suppose now F to be so large, positive or negative, as to make v so great that V may be neglected in comparison with it, then we may think of the cups as whirled round in quiescent air in the positive or usual direction when F is negative, in the negative direction when F is greater than F_1. When F is sufficiently large the resistance may be taken to vary as v^2. For equal velocities v it is much greater when the concave side goes foremost than when the rotation is the other way. For air impinging perpendicularly on a hemispherical cup Dr. Robinson found that the resistance was as nearly as possible four times as great when the concave side was directed to the wind as when the convex side was turned in that direction (*Transactions* of the Royal Irish Academy, vol. xxii. p. 163). When the air is at rest and the cups are whirled round, some little difference may be made by the wake of each cup affecting the one that follows. Still we cannot be very far wrong by supposing the same proportion, 4 to 1, to hold good in this case. When F is large enough and negative, F may be taken to vary as v^2, say to be equal to $-Lv^2$. Similarly, when F is large enough and positive, F may be taken equal to $L'v^2$, where in accordance with the experiment referred to, L' must be about equal to 4 L. Hence we must have nearly—

$$\eta = -L\xi^2, \text{ when } \xi \text{ is positive and very large ;}$$
$$\eta = 4L\xi^2, \quad ,, \quad \text{negative} \quad ,, \quad ,,$$

Hence if we draw the semi-parabola O A B corresponding to the equation $\eta = 4L\xi^2$ in the quadrant η O $-\xi$, and the semi-parabola O C D with a latus lectum four times as great in the quadrant ξ O $-\eta$, our curve at a great distance from the origin must nearly follow the parabola O A B in the quadrant η O $-\xi$, and the parabola O C D in the quadrant ξ O $-\eta$, and between the two it will have some flowing form such as P N M K. There must be a point of inflection somewhere between P and K, not improbably within the positive quadrant ξ O η. In the neighbourhood of this point the curve N M would hardly differ from a straight line. Perhaps this may be the reason why Dr. Robinson's experiments in the paper published in the *Phil. Trans.* for 1878 were so nearly represented by a straight line.

FELLOWSHIPS AT OWENS COLLEGE, MANCHESTER

A SCHEME of Science and Literature Fellowships, modelled very closely after the pattern of the Fellowship Scheme of the Johns Hopkins University, Baltimore, has been organised in Owens College, Manchester. The Council propose, early in October next, to appoint to five Fellowships on the terms and conditions following :—1. The appointment will be made by the Council, after receiving a report from the Senate, not on the results of examination, but after consideration of documentary or other evidence furnished to them. 2. Application by persons desiring to hold these fellowships must be made, in writing, on or before October 1. In his application the candidate should indicate the course of his previous reading and study, and his general purposes with reference to future work. 3. The candidate must give evidence of having received a sound and systematic education either in literature or in science, such as the possession of a degree of an English University, or a certificate from the authorities of an English School of Medicine or Science, of good repute, showing that he has passed through his curriculum with distinction, or, in default thereof, such other evidence as shall be satisfactory to the Council that he is qualified to prosecute some special study or investigation in the manner indicated in § 6. Finally, he should produce a satisfactory testimonial of character and conduct, and should give the names of not more than three persons from whom further information may be sought. 4. In the award of the Fellowships regard will be had to the pecuniary circumstances of the candidates. 5. The value of each Fellowship will be 100*l.* for the academical year 1881-82. In case of resignation or other withdrawal from the Fellowship, payment will be made for the time during which the Fellowship may have been actually held. 6. Every holder of a Fellowship will be expected to devote his time to the prosecution of some special study, with the approval of the Council after receiving a report from the Senate ; and before the close of the year to give evidence of progress by the preparation of a thesis, the delivery of a lecture, the completion of some research, or in some other method. He will study under the direction of the Professor of the subject in which he is appointed, and will be required to pay such fees as the Council shall in each case determine. 7. He may be called on by the Council, after report from the Senate, to render some service to the College, either as an occasional examiner or by giving instruction in lectures or otherwise, to students in the College—provided always that he shall not, during his tenure of the Fellowship, hold any regular or salaried post as Assistant Lecturer or Demonstrator in the College—but he may not engage in teaching elsewhere. 8. He must reside in Manchester during the academical year. 9. He may be re-appointed at the end of the Session for a second and, in like manner, for a third year. 10. Candidates are invited to apply for appointment in any one of the following nine departments :—(1) Classics ; (2) English Language and Literature ; (3) History ; (4) Philosophy ; (5) Pure Mathematics ; (6) Applied Mathematics (including Engineering) ; (7) Physics ; (8) Chemistry ; (9) Biology (including Physiology)

SOCIETIES AND ACADEMIES
LONDON

Royal Society, June 16.—" On Stratified Discharges. VI. Shadows of Striæ," by William Spottiswoode, P.R.S., and J. Fletcher Moulton, F.R.S.

One of the most interesting questions connected with the subject of stratified discharges is this : What is the physical, as distinguished from the electrical, nature of the striæ themselves ? Are they, in fact, to be regarded as aggregations of matter possessing greater density than the gas present in the dark spaces, or are they to be considered as indicating merely special local electrical conditions ? The fact of their having a definite configuration, especially on the side which is turned towards the negative terminal of the tube, that of their temperature being higher than that of the dark spaces, the manner in which they are affected by solid bodies, and other considerations, all tend to give support to the view that the striæ are loci of greater density than the dark spaces. Still it can hardly be said that as yet any experimental proof of this has been given sufficiently decisive to decide the question conclusively. And it is in the hope of contributing something towards the solution of this question that the following experiments are submitted to the notice of the Royal Society.

The two terminals of a Holtz machine were connected in the usual way with the two terminals of the tube, so as to produce a stratified discharge. A narrow strip of tin-foil, or a wire, was stretched along the tube opposite the column of striæ. The positive terminal of a second Holtz machine (in practice we used for this purpose a Töppler machine) was connected with the tin-foil, and the negative terminal with one (either) terminal of the tube. An air-spark, or interval across which sparks could pass, was interposed in the part of the circuit between the machine and the tin-foil. The effect of this arrangement was this : In the interval between two sparks the tin-foil and tube became charged like a Leyden jar ; the tin-foil being the outer coating, charged positively, and the gas inside serving as the inner coating, charged negatively. When the spark passed across the interval mentioned above, the jar (*i.e.* the tube) became discharged, and the electricity previously held bound on the two coatings was set free.

When the first (say the "internal") machine was not working, or when it was disconnected, *i.e.*, when no regular discharge was passing through the tube, then, whenever a spark passed at the second (or "external") machine, a negative discharge with its accompanying Crookes' radiation took place from the inside of the tube next the tin-foil, and the opposite side of the tube became covered with a sheet of green phosphorescence (the tube being of German glass).

When, however, other things remaining as before, a discharge from the internal machine was sent through the tube, and a good stratified column was produced, it was found that the green phosphorescence was entirely cut off from the parts of the tube opposite to the striæ, while on the parts opposite to the dark spaces it remained, in the form of phosphorescent rings, as brilliant as before. The experiment was repeated with various tubes with various degrees of strength of current, and with various densities of gas (produced by heating a chamber of potash in connection with the tube). It may be added that when, as is sometimes the case, through greater exhaustion, the striæ became feebler in illumination and less compact in appearance, the shadows cast by them lost proportionally in sharpness of definition and in completeness of extinction of the phosphorescent light.

The brilliancy and definition of the phosphorescent rings may be increased by inserting a small Leyden jar in the circuit, care being taken that the jar shall discharge itself completely each time. If this is not the case the main discharge is followed by